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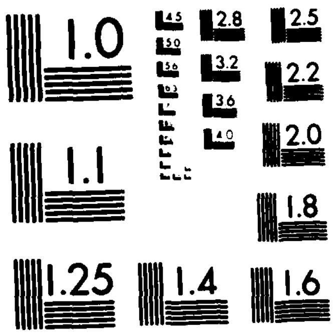

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The Role of Elaborations in Learning a Skill from an Instructional Text

Lynne M. Reder

Davida H. Charney

Kim I. Morgan

Carnegie-Mellon University

Technical Report No. 1
August 1, 1984

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This paper examines the role of elaborations in learning a skill from an instructional text. Two sources of elaborations are compared: those provided by the author and those generated by the learners while reading. In two studies, we tested the effectiveness of author-elaborated and unelaborated versions of a manual in terms of how well they helped subjects acquire and perform a cognitive skill (learning to use a personal computer). In addition to manipulating the availabilitv of author-generated elaborations, we also

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manipulated how effectively subjects could generate elaborations. Some subjects were given advance information about the tasks they would have to perform so that they would generate more specific, task-related elaborations while reading. Elaborations facilitated performance, regardless of whether they were author- or reader-generated. One source was sufficient, however; author-provided elaborations helped only when subjects had no advance information about the tasks. Indeed, subjects with both sources of elaborations performed worse on skill-performance tasks than subjects with only one. We explain this performance deficit in terms of ineffective strategies for coping with an increased cognitive load within a limited study time.

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Abstract

This paper examines the role of elaborations in learning a skill from an instructional text. Two sources of elaborations are compared: those provided by the author and those generated by the learners while reading. In two studies, we tested the effectiveness of author-elaborated and unelaborated versions of a manual in terms of how well they helped subjects acquire and perform a cognitive skill (learning to use a personal computer). In addition to manipulating the availability of author-generated elaborations, we also manipulated how effectively subjects could generate elaborations. Some subjects were given advance information about the tasks they would have to perform so that they would generate more specific, task-related elaborations while reading. Elaborations facilitated performance, regardless of whether they were author- or reader-generated. One source was sufficient, however: author-provided elaborations helped only when subjects had no advance information about the tasks. Indeed, subjects with both sources of elaborations performed worse on skill-performance tasks than subjects with only one. We explain this performance deficit in terms of ineffective strategies for coping with an increased cognitive load within a limited study time.

An important question to both memory theorists and pedagogists is what variables will improve the learning and retention of written information. One such variable that has been the topic of considerable speculation and research is the effect of elaborations (Anderson & Reder 1979; Reder 1976; Reder 1979; Weinstein 1978; Mandl & Ballstaedt 1981; Mandl, Schnotz & Tergan 1984; Bransford 1979; Chiesi, Spilich & Voss 1979; Craik & Tulving 1975; Reder (in press)). In the view of most researchers, there are several reasons why elaborations should help subjects learn and remember the main ideas of a text. Elaborations provide multiple retrieval routes to the essential information by creating more connections to the learner's prior knowledge. If one set of connections is forgotten, it may be possible to retrieve the desired information another way. Further, if the learner forgets an important point, it may be possible to reconstruct it from the information that is still available.

For the purposes of this paper, we define elaborations as pieces of information that support, clarify, or further specify the main points (or gist) of a text. Elaborations take many forms including examples, details, analogies, warrants, restatements and deductions. Elaborations can arise from two distinct sources: first, the text itself can contain elaborations of the main ideas and second, the reader can generate them independently while reading. Each source has its own merits and drawbacks. Elaborations provided by the author of the text may be more accurate than those the reader can come up with, since the author is presumably more knowledgeable about the topic. On the other hand, the reader's own elaborations are likely to be more relevant to his or her immediate purpose for reading, and also more memorable (Bobrow & Bower 1969; Rohwer & Ammon 1971; Rohwer, Lynch, Levin & Suzuki 1967, Rohwer, Lynch, Suzuki & Levin 1967).

There has been ample research supporting the idea that reader-generated elaborations facilitate retention. This support comes from experiments where subjects have additional knowledge that allows them to generate more elaborations than other subjects. Some experiments contrast subjects with a lot of domain-relevant knowledge (for example, about baseball), to those with little relevant knowledge (e.g., Chiesi, Spilich & Voss 1979; Arkes & Freedman 1984). Other experiments provide some of the subjects with additional information that is relevant to a passage to be read. For example, Bower (Bower, Black and Turner 1979) told some of his subjects that the passage's protagonist suspected that she was pregnant; Sulin and Dooling (1974) told some subjects that the passage was about Hitler rather than Gerald Martin. Brown et al (1977) instructed half of the subjects about the fictitious Targa tribe a week before they read about it in the stimulus passage. Subjects who have more relevant knowledge are more likely to intrude not-presented but relevant information and are also more likely to false alarm to plausible inferences based on this additional information. Therefore it is reasonable to conclude that these subjects are in fact elaborating on the presented material with their relevant prior knowledge.[1] More important from our perspective, these subjects also show significantly better retention for the gist of the material and better understanding for it (Anderson & Pichert 1977; Arkes & Freedman 1984; Bartlett 1932; Bower 1976, Brown et al 1977; Dooling & Cristiaansen 1977; Owens & Bower 1977; Schallert 1976; Weinstein 1978).

Although there is considerable support for the idea that reader-generated elaborations facilitate retention, the evidence from author-provided elaborations indicates that they are not as beneficial to learning as expected. Indeed, most of the research suggests that author-provided elaborations hurt retention of the central ideas . Reder

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[1] Dooling & Cristiaansen (1977), for example, explored to what extent the false alarms were due simply to response bias as opposed to differential encoding. Although part of the effects are due to response bias, part of the result is clearly due to encoding differences.

and Anderson (1980, 1982) conducted ten separate experiments on author-provided elaborations and found, to their surprise, that subjects who studied fully-elaborated chapters taken verbatim from standard college textbooks performed consistently worse on recall and comprehension tests than subjects who studied summaries that were one-fifth as long. No matter whether the retention interval was 20 minutes, 30 minutes, one week, or 6-12 months, the summary condition showed a consistent advantage on true/false tests and on short answer tests. The same pattern emerged for reaction time differences as for percentage correct. The advantage held over several kinds of tests: Summaries were superior for questions taken directly from the text and for inference questions that required the subject to combine studied facts. Allwood, Wikstrom & Reder (1982) found the same advantage for summaries in a free recall test. In a transfer task, subjects learned new information better if information learned earlier on a related topic was acquired by reading a summary. Mandl, Schnotz and Tergan (1984), who also found an advantage for summaries, attributed it to the readers' inability to pick out the important points in the elaborated texts. However, in a study designed to facilitate such identification, Reder and Anderson (1980) underlined the main points in the elaborated texts, but the summaries still maintained their advantage. Reder (1982) even found the summary advantage to hold in non-laboratory situations where subjects took the material home to study for as long as they desired. The pervasive finding seems to be that, especially with a fixed study time, people learn facts best when they study those facts *without* elaborative information.

Not all research on author-provided elaborations has found them to impair learning. However, the conditions under which such elaborations benefit the learner tend to be rather specialized. For example, Stein and Bransford (1979) studied subjects' recall of an adjective cued by the sentence frame within which it had been studied. The elaborations in these cases were additional phrases or clauses that heightened the importance of the adjective to the plausibility of the sentence. For example, subjects could better recall the adjective "tall" to the cue "The ___ man took the box of crackers," if the phrase, "from the high shelf" had also been studied.

Bradshaw and Anderson (in press) attempted to devise especially related elaborations that would facilitate recall of an entire sentence (cued by the sentence subject). For a fixed amount of study time, the best they could do was to get equivalent performance in the elaborated condition with learning in the isolated (unelaborated) condition.

Mandl, Schnotz and Tergan (1984) found that the subject's prior knowledge interacts with the usefulness of elaborations. Elaborated texts facilitated recall and comprehension performance only if the reader was very knowledgable in the topic area. For less knowledgeable subjects, the elaborated texts produced worse performance than unelaborated.

Rothkopf and Billington (1983) found that elaborated passages were sometimes superior to unelaborated passages, but only when they were mixed into the same texts; when subjects read one text that was uniformly elaborated and another that was summarized, the summarized version produced better retention of the central points. Presumably, the former result for the mixed passages was due to a signalling problem. That is, subjects assumed the unelaborated parts of the text were less important, and therefore subjects paid less attention to the shorter segments. Indeed, Glover has found similar results[2].

The finding that author-provided elaborations are often ineffectual and sometimes even detrimental to learning is a serious and curious charge. The implications for textbook production would be grave if one actually *believed* the result. One factor that we believe is crucial to the effectiveness of the elaborations found in instructional texts is the kind of learning that is expected to take place. Indeed, educators and laymen alike will often assert that it is much less important to know a set of facts than to know *how to use* these facts. A similar distinction between fact learning and skill learning has received considerable attention in recent years in the cognitive science literature under the label of declarative versus procedural learning.

We believe that many studies found elaborations to impair learning because their tests of learning demanded only the retrieval of specific facts rather than the application of facts to a skill. Most of the research on the role of elaborations in memory and learning has either focused exclusively on declarative knowledge or it has ignored the declarative/procedural distinction. As a result, this research has primarily used verification or recall as measures of mastery, tests which measure the ability to retrieve information but not the ability to "use" the information in the sense of determining the appropriate contexts of application. In the next two sections, we will briefly contrast the nature of each kind of learning and explain why we expect author-provided elaborations to play a

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[2]In other unpublished work of his, Glover and his colleagues (Phifer, McNickle, Ronning & Glover 1983) found that summaries produced worse performance than the elaborated texts if subjects were given approximately one-fifth the reading time for the summaries than they got for the elaborated text. In this experiment, subjects were timed reading sentences from a novel and then had to read the summary sentences at the same average rate. Under these circumstances, the subjects could not comprehend the summaries, and their performance suffered. Reder and Anderson (1982) used a different method of equating reading time per presented sentence for the elaborated and summary texts, but with a slower overall presentation rate. This time, subjects in both conditions spent the same amount of time studying the main points, but those in the elaborated condition spent additional time studying the elaborations. Under these conditions, performance was much better without the "benefit" of the elaborations.

different role in each.

Why do author-provided elaborations impede fact learning?

There are two characteristics of tests of factual knowledge that may lead a person to perform better after studying a summary than a full, elaborated text. First, tests which ask subjects to recall or recognize studied statements require retrieval of specific facts learned at a particular time. Some of these tests also require a minimal amount of inferential processing of the retrieved facts. Good performance on these kinds of tasks results from strong memory traces of the specific facts that must be rieved. The stronger the trace the more likely that it will be retrieved at test. A pror ion or fact is strengthened in memory to the extent that the subject devotes moı a ntion to it. Studying summaries facilitates performance on tests of factual knowledge, because it allows the reader to devote full attention to the essential facts, exactly those that must be retrieved at test.

Under this analysis, studying elaborated texts impedes learning the main points of the text because reading the elaborations reduces the amount of time subjects can devote to the main points. The Total Time Law is a well established verbal learning phenomenon (e.g., Bugelski 1962; Cooper & Pantle 1967); however, it can not completely explain the advantage of the unelaborated versions. In one experiment of Reder and Anderson (1983), study time was equated for main points, i.e., these facts were presented for equal amounts of time in isolation on a computer terminal. The only difference was that in one condition, subjects also studied additional related facts after studying each main idea. Performance was significantly worse with the additional facts. We therefore suggest that the absence of interference also plays a significant role in the superiority of the summary conditions.

Interference differs from the Total Time explanation in that it affects retrieval rather than encoding. There is ample evidence for the existence of retrieval interference on both recall (e.g., Postman 1971; Postman & Stark 1969) and on response times to verification (e.g., Anderson 1974; Reder & Anderson 1980). The elaborations not only take study time away from the important points, they also interfere with accessing these points when they are needed later.

Another reason that elaborations do not help performance in factual tests is that there is little uncertainty in the testing situation about how to *apply* the knowledge. In a recognition or a recall test, it is usually clear what information is needed, when or why to retrieve it and what to do with it once it is retrieved. The information found in an elaboration is seldom called on in such tests and therefore only distracts the reader, when his or her time would be better spent studying the targeted facts themselves.[3]

In short, because of the specific demands that tests of fact learning place on the retrieval processes, studying summaries focuses subjects' attention in just the right places to produce the best performance.

Why might author-provided elaborations facilitate skill learning?

The situation in tasks requiring skill performance is quite different. The application of knowledge to a task of skill can require complex judgments of the appropriateness of the stored information to the context -- judgments that are not required in the simple memory tests that have heretofore been used to demonstrate the superiority of summaries. We are speaking here of acquiring a cognitive skill that the learner can

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[3]Essay exams do not fall into the category of declarative tests as we are defining them. Writing an essay clearly calls for a deep understanding of a body of information and for selecting appropriate items from among the relevant facts.

apply to a variety of novel situations. This type of skill contrasts quite strongly with the kind of skill required to follow a set of instructions for assembling a device or a piece of machinery. The skills for "assembly" do not pose problems of application. Typically, there is usually only one correct way to follow the instructions and there is little to memorize since one usually carries out the instructions immediately after reading them. On the other hand, the cognitive skills we are investigating (namely, learning to manipulate files and directories on a personal computer), can be used much more generally. For example, the command for copying a file can be used to accomplish numerous purposes, and the most correct (or most efficient) way of using the command depends entirely on the situation at hand. Our manual teaches the general procedure for copying files, but it cannot deal explicitly with every possible variation of the command. Furthermore, there may be more than one command or combination of commands that will accomplish the user's goal, so it is much harder to determine which procedure should be applied when.

We believe that studying carefully chosen elaborations, such as examples, will help a learner successfully solve novel skill-related problems. Rather than distracting the reader away from the main points, the information in the elaborations may make it easier to decide which procedures are appropriate to the situation. For example, Ross (1984) has found that subjects choose between equivalent text-editing procedures on the basis of the similarity between the nominal situation at test and the situation evoked in examples found in the materials used to teach the procedures. Further, when no studied procedure is exactly right for the situation, elaborations can make it easier to modify the contexts of application of a previously learned procedure to make it fit the current situation, or use a combination of separate procedures.

In our task domain, learning to use a personal computer, we expect examples and other elaborations to play an important role for helping subjects determine how to issue specific commands to the computer, as well as deciding when a particular command or strategy is appropriate. Our manuals teach subjects how to manipulate on-line files and directories using DOS, the Disk Operating System on the IBM Personal Computer. During the criterion test, subjects must determine which command or commands to use to achieve a set of specified goals. In order to carry out their plans, they must remember the exact names of the commands they want to use as well as the specific syntax of the commands. (e.g., how many parameters are required and in what order they appear). Even though both the elaborated and unelaborated versions of our manuals contain all the information necessary to complete all of the tasks, we expect the elaborated version to help subjects complete more tasks, more efficiently. We expect examples of syntactically correct commands to help subjects remember their names and syntax more easily, and to help them induce the correct commands to select and the most efficient manner to apply them to a task.

In summary, we suspect that the findings of poorer performance for elaborated tests reflect the minimal demands that tests of declarative knowledge make on the learner's understanding of the text. Although elaborations may impair learning of the main points of a text by competing with them for study time and interfering with their retrieval, we believe that the usefulness of elaborations will manifest itself in more sophisicated tests of knowledge. Specifically, we expect the benefit of the additional information to outweigh the liabilities of less study time for the main points and retrieval competition.

The effect of reader-generated elaborations on skill-learning

The research cited earlier on reader-generated elaborations dealt exclusively with their effects on declarative learning. Little is known about the effect of reader-generated elaborations on skill learning, but there is reason to believe that they may not be as trustworthy as those the author can provide. When the reader must draw inferences, invent examples and anticipate consequences of actions in an unfamiliar domain, the resulting elaborations may very well be incomplete or inaccurate. The reader may be led astray by prior misconceptions or superficial similarities. On the other hand, the elaborations a reader generates are more likely to be relevant to that reader's immediate goals. To the extent that these elaborations are more easily integrated with the reader's prior knowledge, they may also be more memorable.

In an attempt to control the degree to which subjects could generate effective elaborations, we provided half of our subjects with information about the tasks they would have to perform before they studied the user's manual that served as our instructional text. We expected that as these subjects read the manual, they would focus on those parts that seemed relevant to the task, generate useful, task-specific elaborations and ignore anything in the manual that did not seem task-related.[4]

We expect reader-generated elaborations to improve subjects' performance, but it is unclear how they will compare to author-provided elaborations and how they will interact. Are subject-generated elaborations more or less important in skill learning than author-provided elaborations? Does the presence of one source of elaboration compensate for the lack of the other? Are the effects additive or is one source enough? Do they

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[4]Providing prior knowledge of the test material has been explored extensively in test of declarative memory. See R.C. Anderson & Biddle (1975), Reder (1982), Reder (in press) and Rickards (1979) for reviews.

interfere with each other?

Experiment 1

In this experiment, subjects were asked to read one of two versions of a user's manual (that we wrote) on how to use the IBM Personal Computer (IBM-PC). We then measured their facility at using an IBM-PC without the manual when they were given specific tasks to perform on the machine.

Method

Subjects

Forty-five members of the Carnegie-Mellon University community, 33 students, 3 faculty, and 9 staff members participated in this experiment. All subjects were occasional to frequent users of C-MU's DEC-20 computer system. Three subjects were non-native speakers of English, but fluent enough to completely understand the documentation. Subjects received either money or class participation credit or a combination of the two. A subject could receive either $8.00, $4.00 plus one credit, or 2 credits (out of a required 3 credits) for participating.

Design

The experiment used a 2x2 between-subjects factorial design, where the first variable manipulated whether the document contained elaborations or not, and the second varied whether subjects read the task instructions prior to studying the document or not. Subjects were randomly assigned to conditions, with the constraint that ten subjects be assigned to each of the four conditions. Although we noted how much previous computer experience our subjects had, we did not control for this variable in assignment to conditions, except for ensuring that no subject had ever worked on a microcomputer. Rather, we used prior experience as a covariate in our data analyses.

Performance was measured with both a procedural test (actually asking the subjects to do something on the computer) and a surprise declarative test (completing a paper and pencil questionnaire). These measures are described in detail in the Results section.

Materials

Documents. Two versions of a user's manual were developed for teaching novices to use the DOS operating system on an IBM personal computer. Both versions were constructed by modifying the official documentation published by IBM. The same main points were presented in both versions; they differed only in the degree of elaboration of the conceptual information and the commands described. The manuals were divided into two sections. The first section discussed concepts underlying the IBM-PC and its operating system. These topics included disk drives, directories and subdirectories, and the use of wildcard characters. This section also contained general information about how to use the machine, how to use the manual, how to issue commands to the machine, etc. The first section was intended to prepare the subject to give the commands specified in the second section of the document, since issuing commands often requires specifying the location of a file in terms of the disk drive the file is on, and a path to the file through a network of directories and subdirectories.

The second section of the documentation contained information about the eleven commands that we taught our subjects. A complete list of these commands is provided in the Appendix. When describing or introducing a command, the manual explains what the command does, what parameters must be specified when the command is issued, any optional parameters, and any other special information about it. The descriptions of the commands also contain options for modifying the commands, if any, and any special information about that command. For example, the section on the DIR command describes an option for producing a more compact display, and an option for making the display pause when the computer screen is full. Several sections include information on how to use wildcards with particular commands. Table 1 gives examples of documentation from the first section and from the second section for both versions of the documentation.

The unelaborated version of the document was constructed by deleting portions of the elaborated manual, such as examples, analogies, metastatements, and definitions. Table 2 gives samples of each of these types of elaboration from the elaborated manual, with the corresponding sections of the unelaborated manual printed alongside. The elaborations did not contain any information that would be needed to successfully complete the tasks we asked subjects to perform.[5] The elaborated version contained approximately 11,200 words, while the unelaborated version contained approximately 5,000 words.

[5]Clearly the specific nature of the elaborations plays a role in how useful they are to learning and to performance on specific tasks, however, for the present, we are relying on intuition to help us select the most useful elaborations we can, primarily examples and illustrations. We will discuss later the issue of how different types of elaborations might manifest differents effects on subject behavior.

Table 1

Excerpts from the elaborated and unelaborated versions of the manual used in Experiment 1

Section 1: Basic concepts

ELABORATED

GIVING THE COMPUTER A COMMAND

In order for the computer to store information or perform a calculation, you have to give it a command which specifies exactly what you want it to do. To give a command, you type words and symbols in a fixed format that the computer recognizes. The commands you will give the computer consist of three parts: the name of the command, the command's "parameters," and a carriage return <CR>. The computer "prompts" you when it is ready for you to type a command by displaying either A> or B> on the screen. Throughout this book this "prompt" is highlighted so you will remember that the computer types this, not you.

For example, the following command tells the computer to find a file called REPORT.MSS, and type the contents of this file on the compuer screen:

A>TYPE REPORT.MSS <CR>

CMD NAME. The name of the command is usually an English word or abbreviation, *such as TYPE in the example above. The computer uses the name of the command to look up a program, a set of instructions for carrying out the command.* The commands that the computer recognizes are discussed individually later in this book. *The computer recognizes some abbreviations of the command names, such as REN for RENAME, and the accepted abbreviations will be listed with the description of each command.*

UNELABORATED

GIVING THE COMPUTER A COMMAND

In order for the computer to store information or perform a calculation, you have to give it a command which specifies exactly what you want it to do. To give a command, you type words and symbols in a fixed format that the computer recognizes. The commands you will give the computer consist of three parts: the name of the command, the command's "parameters," and a carriage return <CR>. The computer "prompts" you when it is ready for you to type a command by displaying either A> or B> on the screen. Throughout this book this "prompt" is highlighted so you will remember that the computer types this, not you.

The general format of a command statement looks like this:

A> [CMD NAME] [PARAMETER] <CR>

CMD NAME. The name of the command is usually an English word or abbreviation. The commands that the computer recognizes are discussed individually later in this book, along with acceptable abbreviations for each one.

Table 1, continued

Section 2: Specific commands

ELABORATED	UNELABORATED
CHANGE THE NAME OF A FILE: RENAME	CHANGE THE NAME OF A FILE: RENAME
Use the RENAME command to change the name of a file. You may want to change a file's name to make it easier to remember or to make it similar to the names of other files that contain related information. You can use RENAME to change the stem part of a filename, its extension, or both.	Use the RENAME command to change the name of a file.
FORMAT	**FORMAT**
The format of the command is:	The format of the command is:

RENAME [loc & name of file to be renamed] [new name of file]

ELABORATED	UNELABORATED
You can use the abbreviation REN in the command instead of typing RENAME.	You can use the abbreviation REN in the command instead of typing RENAME.
[Loc & name of file to be renamed] refers to the path to the file you want to rename. Type the name of the file as the last name on the path.	[Loc & name of file to be renamed] refers to the path to the file you want to rename. Type the name of the file as the last name on the path.
[New name of file] is what you want to call the file from now on. You simply type the new filename. You do not have to supply any location information, since the renamed file stays in the same place it was before.	[New name of file] is what you want to call the file from now on. You simply type the new filename. You do not have to supply any location information, since the renamed file stays in the same place it was before.
For example, the following RENAME command specifies that the file BUDGET should be renamed BUDGET.83:	
A> RENAME B:BUDGET BUDGET.83 <CR>	
The path indicates that BUDGET is located in the current directory on drive B. The new name for the file, BUDGET.83, is not preceded by any path information.	

Table 2

Samples of four types off elaboration

from the manual used in Experiment 1

Meta-statement

ELABORATED

Since the computer has two disk drives which can each contain a diskette, you must specify whether the file you want is in drive A or drive B when you give the computer a command. If your command doesn't specify which drive contains the file, the computer automatically assumes that it can find the file in the "default" drive. The next section explains what the "default drive" is, and how to tell the computer to look on a different drive if necessary.

UNELABORATED

Since the computer has two disk drives which can each contain a diskette, you must specify whether the file you want is in drive A or drive B when you give the computer a command. If your command doesn't specify which drive contains the file, the computer automatically assumes that it can find the file in the "default" drive.

Definition

ELABORATED

The B: ("B-colon") in the command stands for the right-hand disk drive. The colon signals the computer that the letter or word preceding it is a "device" rather than the name of a command or file. Devices are pieces of computer hardware, such as disk drives, a printer or even the keyboard. After you enter the command, the B> prompt will appear on the screen. From now on the computer will automatically look for files on drive B.

UNELABORATED

The B: in the command stands for the right hand disk drive. From now on, the B> prompt appears on the screen and the computer will automatically look for files on drive B.

Table 2, continued

Analogy

ELABORATED	UNELABORATED
When you give the computer a command concerning a file, such as TYPE, ERASE or COPY, the computer looks for the file on a "diskette." A diskette, also known as a "floppy disk," is similar to a small, flexible phonograph record record, except that instead of storing sounds, it contains information which the computer can read, add to or delete. All the files you create on the computer are stored on diskettes. So, in order to work on your files, you must insert the diskette that contains them into the computer. You insert a diskette into one of the two "disk drives" on the front of the computer cabinet. The drive on the left is called drive A, and the one on the right is drive B.	When you give the computer a command concerning a file, such as TYPE, ERASE or COPY, the computer looks for the file on a "diskette." To use a diskette, you insert it into one of the two "disk drives" on the front of the computer cabinet. The drive on the left is called drive A, and the one on the right is drive B.

Table 2, continued

Example

The elaborated and unelaborated versions are for the most part identical except for the addition of the example (italicized here)

Using COPY to Combine Files

You can use COPY to combine files, appending a copy of one file to the end of another file.

FORMAT

The format of the command is:

COPY [Loc & name first file+next file+...] [Loc & name combined file]

[Loc & name first file+next file...] refers to a list of the files you want to "add" together. The names of the files are typed with plus (+) signs between them. You need to specify location information for each filename in the list in the usual way, with drive and path specifications. When several filenames are listed in this manner, the COPY command results in a new file in which the contents of the first file on the list appear first, followed by the contents of the second file, then the contents of the third file and so on. So be sure that the files in the list appear in the order in which you want them combined.

[Combined file] refers to the new file that will contain the combined files; what you want to call this file, and where in the directory structure you want it to go. Specify the location in terms of a drive and a path to a directory as usual. Type the name you would like to give the file at the end of the path.

For example, suppose you write a report in sections, with each section in a separate file. You want to format and print the report as one file, so you combine the sections into one file. The following command takes three files, INTRO.MSS, BODY.MSS, and CONCL.MSS and combines them into a new file called REPORT:

A> COPY B:INTRO.MSS+B:BODY.MSS+B:CONCL.MSS REPORT <ENTER>

The combined file, REPORT will consist of the introduction, the body and the conclusion.

Test materials. Two tests were designed to cover the material presented in the manual: one procedural, the other declarative. The procedural task tested subjects' ability to use 8 of the 11 commands on line, from memory. This task will be described in more detail in the procedure section. The paper-and-pencil declarative test covered all 11 commands, including some special uses of the same commands that were required in the procedural task. Half of the declarative knowledge questions required the subject to write a command that would produce a specified result if issued on the computer. The other half of the questions required that the subject describe (in writing) what effect a specified command would have in a specified context. The commands taught in the manual were randomly assigned to either the "generate command" or the "describe result" question type. An example of each type of question is provided in the Appendix. There were 11 questions in all.

Apparatus

Two IBM Personal Computers, each with two floppy disk drives, were used in running the experiment. The PC's stood on nearby tables and were connected together by a cable between their serial ports. Software was developed to collect protocols of the subject's interactions with the computer. While the subject sat and issued commands at one PC, the commands and the computer's responses were echoed across the cable and recorded (with a timestamp) in a file on the second PC. A room-divider blocked the subject's view of the second PC's screen.

Procedure

All subjects saw the following instructions, either before or after studying the user's manual:

Before you, in drive A, is a diskette containing a number of files. These files have arbitrary meaningless names, such as "F1." We want you to inspect each file and rename it according to the name listed in the first line of the file.

After renaming each file, we would like you to create a number of subdirectories. Name each sub-directory according to the files that will be stored under it. You should sort the files into subdirectories on the basis of their "stem" names. That is, if you had a file named "foo.pas", you would sort it by "foo," not by "pas". Put all files with the same stem into a sub-directory bearing that name.

If there are files that have a unique stem name, i.e., no other file has the same stem name, then we want you to "migrate" that file from the current diskette. To migrate the files, you will copy them to the diskette on drive B, and then delete them from the diskette on drive A. You should also create a file called "migrate.lst" at the top level directory of the diskette in Drive A. Type the names of all the files you migrated into migrate.lst.

To summarize your tasks:

1) Inspect each file to discover its intended name. Rename each file according to the name found on the first line in that file.

2) Create a hierarchical set of directories corresponding to the types of files that you have. Create a sub-directory for each "type" of file, where type is defined in terms of the communality of stem names. Transfer the appropriate files into each subdirectory.

3) For those files that have a unique stem name, do not create a sub-directory. Instead copy those files to the diskette on Drive B, and delete them from the directory on Drive A.

4) At the top directory level on the diskette in Drive A, you should now have a set of subdirectories, but no files. You should create a file in this directory that lists all the files that were copied to the other diskette.

Now, you are done with the tasks!

Those subjects who were randomly assigned to the condition that allowed them to learn the specifics of the task before reading the manual (the Before condition) read the task instructions before being given their version of the manual. The other half of the subjects (the After condition) read through the task instructions for the first time when they were about to perform the task. Half of each of these groups were given the elaborated version to read and half the shorter version.

All subjects, regardless of condition, studied the manual for 45 minutes. They were told that since the manual would not be available while they performed the task, they should review the important points of the manual as often as time permitted. Subjects were told when the reading period was half over. Subjects studied the manual while seated in front of the computer they were reading about. They were told to examine the keys referred to in the documentation; however, they were also instructed not to issue any commands or otherwise touch the machine, and not to take notes. When the reading period was over, the experimenter returned to the room where the subject studied the document to give the subject the task instructions (for the second time if the subject was in the Before condition).

Even though the computer recorded all interactions with the subject, the experimenter remained in the room while the subject attempted to perform the specified tasks. It was the responsibility of the experimenter to determine if the subject had arrived at an impasse, i.e., could not complete part or all of the current task. Either the subject gave up or was stopped after approximately 10 minutes of fruitless effort. At this point, the experimenter replaced the diskette the subject was working on with a prepared diskette on which the procedures for that task or sub-task had already been completed. In this way, the subject could proceed to the next task as if he or she had actually completed the sub-task. The experimenter also took notes on any occurrences that would not be recorded by the computer (such as spoken comments). Subjects were not allowed to ask questions, unless the question was of a superficial nature, e.g., which key was the carriage return. (This was not obvious on the IBM-PC keyboard).

After all the requirements of the task instructions were completed to the subject's satisfaction or abilities, the surprise declarative (short-answer) test was administered. Subjects were allowed to take as long as they wanted to complete this test; however they generally took about 15 minutes. The entire experiment took approximately 1.5 to 2 hours, depending on the subject's ability.

RESULTS

The following results reflect the scores of 40 subjects. In all, 45 subjects completed the study, but the data from five subjects were either thrown out or were unusable. Data from three subjects were lost because of computer failure. Data from two subjects were thrown out because they had so little previous computer experience that they refused to continue the experiment shortly after beginning the procedural task.

Scoring

The protocol of a subject's interactions with the computer were stored in a file which was subsequently analyzed by means of a computer program. The program allowed the experimenter to categorize segments of the protocol according to the subtask the subject was working on. The subjects' task can be divided into three separate sub-tasks:

1. Rename task: inspect the contents of all files on diskette and rename them.
2. Make-directory task: create three subdirectories and move the appropriate files into them from the root directory.
3. Migrate task: ship files from one disk drive to another and create a new file.

The program produced a variety of statistics about each protocol by counting commands and calculating time intervals, either within or across subtask "partitions." For example, the program could determine how many times a subject had issued a "TYPE" command while working on the Rename task, and how much time (in seconds) it took him or her to issue those commands. The partitioning of protocols into "subtask partitions" was carried out independently by two judges for a random subset of the protocols. The agreement between the judges was quite high (the regression coefficients ranged from $r = .98$ to $r = 1.0$), and any disagreements were resolved to mutual satisfaction. (The appendix contains an excerpt from a subject's protocol before and after it was analyzed using this program).

The data were scored in several ways. One dependent measure was simply the *total time* a subject spent attempting to complete the task requirements. Another measure was *total steps*, or the number of commands the subject issued while working on the task. A third measure was a *completeness* measure, viz., how many of the sub-tasks were successfully completed. Because of the large variation in times to "quit" on a sub-task, we also measured how much time subjects took on just those sub-tasks they successfully completed. Since the sub-tasks themselves varied in the average time needed for successful completion, we looked at how a subject's time on a successfully completed task deviated from the mean of times for all successful completions on that task. Then, for each subject, we calculated a mean of proportional times for the tasks he or she completed. Similarly, we computed a subject's mean of proportional steps (i.e., number of steps/avg. number of steps for all subjects who completed this task) for each completed sub-task. Finally, we compared the number of steps subjects took on tasks they completed with the *minimum* number of steps required to complete those tasks.

We developed a completeness scale to score subjects for how successfully they completed each of the three sub-tasks. In order for the Rename task to be complete, each of the 15 files must have been correctly renamed. If a subject correctly renamed some but not all of the files (which never happened), or used the "copy" command (which creates an extra copy of a file), instead of the "rename" command and then failed to delete the version with the old name, the subject received a score of one-half instead of one point.

In the Make-directory sub-task, subjects received 3 points if all three sub-directories were created and all the appropriate files were copied into them and then deleted from the root directory. For any subdirectory that was not properly created or did not contain all the files it should, subjects lost a point.

In the Migrate sub-task, one file had to be copied to the diskette on drive B, erased from drive A, and a new file created which listed the name of that file. The last step involved a special form of the COPY command which enabled the subject to copy data typed at the console keyboard into a file. If each of these three steps was executed, the subject received three points.

Performance on Task

Success on Tasks

The principal dependent measure is the degree to which a subject was able to satisfactorily perform the various tasks specified in the instructions. Table 3 shows the proportion of tasks completed as a function of version of document and whether the instructions were read before the manual or only after. There is a 9% advantage for reading the elaborated version and a 5% advantage for not seeing the task instructions until after studying the manual; however neither of these effects is significant. When subjects did not know what tasks to expect, the difference between the two versions of the manual is almost 20%; however, in the Before condition, there is no difference. Although the interaction is not significant, the superior performance of the Elaborated-After condition compared to the other three conditions is significant, $t(38) = 2.98$, $p < .05$.

This condition is about 15% better than any of the other three, and these three are about equivalent in overall performance. When broken down by subtask, this measure produces the same pattern for all three subtasks, as shown in Table 4. The *rename* task is so easy that performance is on the ceiling; however, for the second and third tasks (creating new directories and moving files to another drive), performance is much better for the Elaborated-After condition, $t(38) = 1.98$ and 2.62, respectively. The former contrast is marginally significant and the latter is significant, $p < .05$.

TABLE 3

Mean proportion of tasks successfully completed

for Experiment 1*

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	.79 (.80)	.79 (.80)	.79 (.80)
	After	.93 (.92)	.75 (.82)	.84 (.87)
		.86 (.86)	.77 (.81)	

* The numbers in parentheses are the means for each cell adjusted for the number of prior programming languages. See text for further explanation.

TABLE 4

Mean proportion of each subtask
to be successfully completed
for Experiment 1

	ELABORATED		UNELABORATED	
	BEFORE	AFTER	BEFORE	AFTER
SUBTASK				
Rename	1.00	1.00	1.00	.90
Make Directory	.80	.93	.82	.68
Migrate Files	.70	.90	.70	.77

Efficiency of task execution: number of steps

In addition to measuring a subject's ability to perform a task, one can ask whether a subject performed the task in the *optimal* manner. For example, we asked subjects to change the names of a set of files. A name change can be accomplished by using the COPY command to create an additional version of the file with a new name, and then using the ERASE command to delete the version of the file with the old name. However, this method takes more steps to complete than using the RENAME command, which does not involve creating another version of the file. Our scoring procedure allowed us to tally the number of steps a subject took (i.e., how many commands he or she issued) within each subtask. The mean number of steps subjects took to perform all tasks is presented as a function of condition in Table 5a. Here, as with the completeness measure, the elaborated manual produces the best performance, and reading the manual without advance knowledge of the tasks also improves performance. As expected, due to the high variance in the time unsuccessful subjects took to "quit" on a task, none of the contrasts are reliable.

The replacement for the Total Steps measure avoids the problem of artificially high numbers of steps (when subjects persist in following some dead-end path), and artificially low numbers (when subjects quickly realize that they cannot complete the task and give up right away). We did not use the mean number of steps taken per successfully completed subtask because the number of steps required for a subtask is not standard. The mean for a subject could be biased high or low depending on which subtasks were completed. Instead, we computed the mean of the number of steps needed by those subjects who successfully completed each subtask. A subject's score on a subtask became the ratio of the number of steps he or she took divided by the mean number of steps taken to successfully complete that task. So, to arrive at an overall score for each subject, we averaged the proportional scores for those tasks the subject successfully completed.

Table 5b presents the mean proportion of steps taken per completed task. A number close to unity should be considered baseline. As this table indicates, the two Before conditions produce normative performance, while performance in the After conditions varies with the degree of elaboration in the manual: the Elaborated-After condition produces better than average performance and the Unelaborated-After condition produces worse than average performance. Both the main effect of elaboration and the interaction of elaboration with prior task knowledge are marginally significant, $F(1,35) = 3.08$ and 2.91, $p < .10$, respectively. The Elaborated-After condition maintains its status as the best condition; the contrast of Elaborated-After to the other three conditions is significant, $t(38) = -2.65$, $p < .05$.

In addition to comparing subjects' efficiency relative to each other, we also compared the number of steps each subject took to complete a subtask against the **minimum** number of steps required (i.e., the most efficient solution) to complete that subtask. For this measure, a subject's score on a subtask was the ratio of the number of steps he or she took divided by the minimum number of steps required. To arrive at an overall score for each subject, we averaged the proportional scores for those tasks the subject successfully completed.

Table 5c presents the mean efficiency ratio per completed task. The greater the value, the less efficient the performance. As the table indicates, performance is again best in the Elaborated-After condition and worst in the Unelaborated-Before condition, but no effects were significant.

Table 5a

Mean number of steps taken by subjects

to perform all subtasks

for Experiment 1

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	98.7	106.8	102.8
	After	86.7	103.1	94.9
		92.7	105.0	

TABLE 5b

Mean proportion of steps taken per successfully completed task (compared with the mean number of steps taken by those who successfully completed that task) for Experiment 1*

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	1.01 (1.03)	1.01 (.99)	1.01 (1.01)
	After	.87 (.88)	1.15 (1.15)	1.01 (1.02)
		.94 (.96)	1.08 (1.07)	

* The numbers in parentheses are the means for each cell adjusted for the degree of prior computer usage.

TABLE 5c

Efficiency on completed tasks: the proportion of steps subjects took to complete tasks relative to the minimum number of steps required to successfully complete those tasks, for Experiment 1

		VERSION OF MANUAL: Elaborated	VERSION OF MANUAL: Unelaborated	
INSTRUCTION	Before	2.4	2.3	2.4
	After	2.1	2.9	2.5
		2.3	2.6	

Efficiency of task execution: time on task

We also measured how long subjects took to perform a task. Time was measured using the computer-generated timestamps for when a command was issued. This measure is noisy since subjects felt under no time pressure and were not told that we were measuring how quickly they performed the tasks. Table 6a presents the mean time subjects took to do all of the required sub-tasks. as a function of condition. These times include times for sub-tasks that subjects did not successfully complete. There is no difference in total time as a function of whether subjects had read the task instructions prior to studying the manual (the BEFORE vs. AFTER variable). Although there was a 20% difference in time taken to complete the three sub-tasks as a function of which version the subjects studied (40 minutes for the unelaborated version vs. 33 minutes for the elaborated version), this difference was not significant. Again. the fastest condition seemed to be the Elaborated-After condition, although this contrast was also non-significant.

The total time measure suffers from the same biases as those mentioned above. Therefore. we computed the ratio of a subject's successful completion time to the mean of the successful completion times for that task. We took the arithmetic mean of the ratios for each subject. These values are displayed in Table 6b. The pattern of results is in the same direction as the other dependent measures. but these effects are also unreliable.

Table 6a

Mean time (in minutes) taken by
subjects to perform all subtasks
for Experiment 1

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	35.97	39.80	37.89
	After	30.12	40.29	35.21
		33.05	40.05	

Table 6b

Mean proportion of time taken per successfully completed task (compared with the mean time taken by those subjects who successfully completed that task) for Experiment 1*

		VERSION OF MANUAL: Elaborated	VERSION OF MANUAL: Unelaborated	
INSTRUCTION	Before	.99 (1.04)	1.03 (.96)	1.01 (1.00)
	After	.96 (.98)	1.16 (1.15)	1.06 (1.07)
		.98 (1.01)	1.10 (1.06)	

* The numbers in parentheses are the means for each cell adjusted for the degree of prior computer usage.

Effects of Prior Experience on Performance

We did not control for prior experience with computers in this first experiment.[6] We randomly assigned subjects who had had minimal experience with computers to conditions: however, we did note computer background (by having all subjects complete a self-rating questionnaire), and used this information to perform analyses of covariance.

We classified experience on two dimensions: degree of prior computer usage (rare. frequent and know multiple operating systems) and number of programming languages studied (none, one or more than one language). We performed 3 x 3 analyses of variance on three dependent measures: proportion of steps per completed task (as compared to the mean number of steps taken to complete each task), proportion of time taken per completed task and percent completion. Subjects who had studied more programming languages completed significantly more tasks than those who did not know any languages, $F(2,30)=4.10$, $p<.05$. Subjects who rated themselves as having little experience using computers took (marginally) significantly more time than other subjects, $F(2.30)=2.98$, $p<.10$. The effects of degree of computer experience and number of programming languages did not have additive effects for the number of steps needed to complete a task. $F(4,30)=3.05$. $p<.05$. No other effects or interactions approached significance for these dependent variables.

We performed analyses of covariance on all our measures using each covariate. These analyses showed a significant negative correlation between time taken to perform the tasks and rated prior use of computers. $F(1,34)=4.90$. $p<.05$. The regression coefficient was -2.04. There was also a negative correlation between steps taken and

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[6]We tried using subjects who had very little experience: however, they found the task impossibly difficult and uniformly failed the task. Therefore, we only accepted subjects who had some experience with computers, though none with micro-computers.

this variable, $F(1,34) = 4.52$, $p < .05$. This regression coefficient was -5.13. The only other reliable correlation was between the number of programming languages known and proportion of tasks completed. $F(1,34) = 7.22$, $p < .05$, (regression coefficient = .72). The numbers in parentheses in Tables 3, 5b and 6b show the means adjusted for the effects of prior experience. The adjusted mean selected was always from the covariate that had more impact on that analysis. Although there were significant effects due to these covariates, the adjustments were quite slight and did not change the overall pattern of results nor the significance of any result. Thus, although there were effects of prior experience, they were not confounded with assignment of subject to condition.

Declarative Test

The pencil and paper test was administered after subjects completed the on-line test. This test was also administered to a separate group of 16 C-MU students who took this paper and pencil test immediately after studying the manual, and did not do the on-line task. (Half of these 16 subjects read the elaborated version and half read the unelaborated version.)

A scoring system was developed for the two kinds of questions on the declarative test. As described in the Methods section, half of the declarative knowledge questions required the subject to write a command to produce a specified result. The other half of the questions required that the subject describe (in writing) what effect a specified command would have in a specified context. On the command-generating questions, subjects got full credit if they gave the name or an accepted abbreviation of the appropriate command, and gave the right parameters for the command in the right order. Subjects were credited for any single command that would produce the desired effect, i.e., they were not penalized for giving more path information than was absolutely necessary, nor for giving an alternative command if there was one. To receive full credit on the description questions, subjects had to "interpret" each part of the supplied command. For example, if the command created a file in a particular location, the subject had to say both that it created the file and tell where it would appear. (Partial credit was given if some but not all the information was supplied.)

Table 7 displays the results for the paper and pencil test for subjects who had first completed the skill test. The pattern looks very much like the procedural tests: There are no main effects or interactions; however, the Elaborated-After condition was significantly better than the other three, $t(38)=2.26$, $p<.05$. One reason that the paper and pencil test might mirror the skill test is that subjects had already practiced the commands in the first test, biasing performance in favor of the conditions where the correct answers had been practiced. The performance of those subjects who did not first perform on the skill test is relevant to this interpretation. Those subjects who read the elaborated version got 64% correct on the test and those who read the shorter version got only 54% correct. That difference was non-significant.

The fact that performance was 5-10% worse for subjects who did not first perform the skill test is consistent with the interpretation of a bias from prior testing. Still, the difference between the elaborated and unelaborated versions was even bigger (although not significant) for the subjects who only took the declarative test. The reason that elaborations may have helped here while not in previous studies which employed declarative tests is that the paper and pencil test was more a test of procedural knowledge rather than declarative knowledge. [illegible] required subjects to generate the appropriate command in a given context or understand and generate what the computer would produce in a given context. In contrast, the declarative tests in previous studies tended to require simple fact matching. This point will be discussed further in the next section.

TABLE 7

Mean percent correct for paper and pencil test
for Experiment 1

INSTRUCTION		VERSION OF MANUAL: Elaborated	VERSION OF MANUAL: Unelaborated	
	Before	.65	.63	.64
	After	.73	.64	.69
		.69	.64	

Discussion

It would be useful at this point to summarize our findings. Subjects performed skill tasks better if they had studied the elaborated version of the manual. but this advantage obtained only if they had read the manual without knowing what the test of skill would be. For subjects who had prior knowledge of the tasks. the converse was true. They performed well only if they read the unelaborated version of the manual. We expected the elaborated manuals to produce better performance. and that hypothesis was confirmed, albeit weakly. We also expected that having knowledge of the task prior to study would facilitate performance overall. but that did not occur.

At the beginning of this paper, we discussed two sources of elaborations and contrasted author-provided elaborations with reader-generated elaborations. We expected that prior knowledge of the task would allow the reader to generate better and more relevant elaborations and to focus their attention on those aspects of the manual that would be most relevant to the subsequent task. Using these constructs, it is possible to explain the pattern of results we obtained.

The two After cells most closely represent the conditions of the numerous studies on author-provided elaborations cited earlier. We found that subjects consistently performed best in the Elaborated-After condition and worst in the Unelaborated-After condition. Clearly, in the skill-performance domain, subjects perform better when instructional texts contain author-provided elaborations than when they do not.

It is important to bear in mind that subjects performed pretty well with either version of the manual. The subjects in the worst cell, the Unelaborated-After condition, still completed three-quarters of the tasks successfully. Furthermore. the Unelaborated-Before condition was frequently the second-best condition. This provides some independent reassurance that the difference cannot be attributed to the unelaborated version missing any essential information for completing the tasks.

One reason why advance information about the tasks did not produce better performance overall may be that it caused subjects to focus their attention *overly selectively*. That is, they inadvertently ignored too much relevant information. This could occur if subjects thought they understood what was required to perform the specified tasks (and focussed their attention accordingly), but were wrong. One reason the subjects may have made such mistakes is that the task instructions were taken away prior to studying the manual, i.e., subjects were not allowed to refer to the instructions while reading. Subjects might easily have misremembered or misinterpreted the nature of the tasks. Another related problem is that by giving the task instructions before studying the manual and then removing them, we gave subjects in the Before conditions an additional memory load (i.e., trying to remember the task instructions) that subjects in the After conditions did not have. This too could cause inferior processing of the new information.

For the Elaborated-before condition, the extra source of elaborations may ultimately have hampered these subjects who already had a long, elaborated document to read. One simple explanation for the poorer performance in the Elaborated-Before condition (as compared to the Elaborated-After condition) is that it took subjects too long to read the elaborated version of the manual when they were simultaneously trying to generate more task-specific elaborations. There was not enough time to read the entire document thoroughly under these constraints.

Our second experiment was intended to eliminate some of the factors that might have hampered performance in the Before condition. We were interested to see whether the elaborated version would again be superior to the unelaborated version and whether the Before conditions would be uniformly better than the After conditions when some of the problems mentioned above were eliminated. In Experiment 2. subjects were allowed to set their own pace for studying the manual (within a one hour limit). This change was intended to finesse the problem of subjects having too little time to generate task-specific elaborations for the already lengthy elaborated version. We also eliminated the additional memory load and the potential for mis-remembering the nature of the tasks by allowing subjects in the Before conditions to keep the task instructions while studying the manual.

The paper and pencil test was also redesigned to make it a better test of declarative knowledge and less a test of procedural skills. Modifications were also made to the manuals and the task requirements to eliminate problems such as a subject thinking that he or she performed a task successfully when in fact the procedure used was wrong. These changes will be discussed in more detail below.

Experiment 2

This experiment was largely a replication of Experiment 1 with several major modifications: study-time was self-paced, task instructions could be read during the study period as well as before the study period, and the paper and pencil test following the skill test tapped declarative knowledge more than procedural knowledge.

Method

Subjects

Forty-three Carnegie-Mellon University undergraduates participated either for course credit (to satisfy a requirement) or for pay. Subjects received $6 and no credits, or $3 and 1 credit, or 2 credits for participating. Subjects were screened before being allowed to participate, in order to insure that all subjects were fairly familiar with CMU's Dec-20 computer (which is used regularly by the entire campus community), and had taken no more than one semester of a computer programming course. No subject had ever worked on a microcomputer.

Design and Materials

The design was the same as in Experiment 1 except that we did not administer the paper and pencil test to any subjects who had not already completed the on-line task. Subjects were again randomly assigned to the four conditions defined by orthogonally crossing two factors: degree of elaboration (elaborated vs. unelaborated) and when knowledge of task was acquired (before vs. after studying the manual).

The manuals were modified slightly, primarily by deleting redundancy. For example, in the manuals used in the first experiment, an explanation of how to specify a path through the directory structure was repeated whenever a new command was introduced. In this experiment, the definition of a path was explained once. When a path was used as part of the parameters that define a command, reference was made to the section that explained paths in greater detail. The shorter version was also abridged by deleting even more information that was deemed to be elaborative, e.g., reasons, consequences, restatements. The elaborated version in Experiment 1 contained 11,216 words: after modification, the corresponding version in Experiment 2 contained 10,605 words. Similarly, the unelaborated version which contained 5,019 words in Experiment 1, now was reduced to 3,542 words. So, in Experiment 1 the ratio of the longer to the shorter version was approximately 2:1. In this study, the ratio became closer to 3:1.

There were also a few modifications in the tasks required of subjects. In the first experiment, it was possible for subjects to successfully complete the tasks without having to negotiate their way through the directory structure. One modification of the tasks in this study was to force subjects to draw on what they learned about subdirectories by specifying paths to subdirectories in their commands, or changing the default directory to a subdirectory. Another modification was to reduce repetitiveness within a task. For example, subjects did not have to rename as many files as in the first study The tasks are described in more detail in the Procedure section.

The paper and pencil test bore little resemblance to the one used in the first experiment. In Experiment 1, the subject was asked to either generate a command that would produce the described result, or to describe what the result would be of issuing a particular command in a particular context. Both types of questions required subjects to apply their general knowledge about commands to specific contexts in ways very similar to what was required for the nominally more "procedural" on-line task. In Experiment 2, we wished to make the pencil-and-paper test tap declarative knowledge more directly.

The new test consisted of 24 true/false questions pertaining to the concepts in the manual. We believe that these questions required less ability to recognize contexts of application of a fact, and depended more on the ability to retrieve studied facts from memory. (Sample questions from this test are provided in the Appendix). In order to reduce the possibility that prior knowledge would affect performance on the test, we discarded any question that was not judged at chance levels by subjects who had not read a manual. We asked 43 people to answer the questions and discarded any question whose accuracy was below 25 or above 75 percent correct.

Procedure

The procedure for this experiment differed slightly from that used in Experiment 1. Those subjects assigned to one of the Before conditions were again given the task instructions to read prior to studying the manual; however, this time they were allowed to keep the instructions during the study phase. Subjects assigned to the After conditions were told that they would read a manual about using the PC and would then be given a series of tasks to do on the computer.

Both groups read the manual in front of the PC. Unlike the first experiment in which all subjects studied the manuals for 45 minutes, subjects were now given up to one hour in which to read the entire manual. Subjects were instructed to notify the expermienter when *they* felt they had mastered the manual and were ready to proceed to the task. The experimenter informed the subjects when half the hour had elapsed and recorded how long subjects spent studying the material. Since subjects would not have the manual available during the skill test, subjects were advised to review the important points of the manual until they felt they had mastered them or until the one hour time period had elapsed.

When subjects finished studying the material, the experimenter returned to the room, took away the manual and, in the case of the After conditions, gave subjects the task instructions. The experimenter remained in the room while subjects worked, watched for impasses and noted any interesting strategies that would be missed by the on-line recording of the human-computer interaction. This time the task instructions read as follows:

Instructions

These tasks will allow you to practice using the concepts you learned from the manual. You may work on these tasks in any order. Continue working until you are satisfied that you have completed the tasks to the best of your abilities. We want you, however, to work as efficiently as possible.

Task 1. Before you, in drive A, is a diskette containing a number of files. Some of these files have the word "PART" in their names, such as file "PART.1." We want you to change the names of these files. The new name that you should give each file appears as the first line of that file. So, inspect the contents of each file that now has "PART" in its name and give the file the name that you find on the first line of the file.

Task 2. Four of the files on the diskette have the word "DATA" in their names, and the abbreviation of a month in their extension, such as DATA.MAR. We want you to create a fifth data file named ALLDATA.83 that contains the contents of the other four data files appended together. Within ALLDATA.83, the files should appear in "chronological" order: that is, the contents of DATA.MAR should precede the contents of DATA.JUN because March is earlier in the year than June.

Task 3. Next, you should create two subdirectories, on the diskette in drive B. One subdirectory is to be named PROGRAMS and the other named DATA. Move all the files that have the word "Program" in their names from drive A into the PROGRAMS directory on drive B. And, similarly, move the "Data" files (including ALLDATA.83 from Task 2, if you have already created it), into the DATA directory. You do not want any Program or Data files to remain on the diskette in drive A.

Task 4 Finally, you should eliminate the SOURCE directory and everything it contains from the root directory of the diskette in drive A. The root directory on drive A should now contain only a list of files.

Your task is complete!

After the four tasks were completed to the subject's satisfaction, the surprise declarative test was administered. Subjects were allowed to take as long as they wanted to complete this paper and pencil test; however they generally took about 10 minutes. The entire experiment typically took between 1.5 hours and 2 hours.

Results

Data were analyzed from 40 subjects, although 43 subjects actually participated. Data from three subjects were excluded from the analyses, two due to computer failure during on-line data collection and one due to a subject in the Unelaborated-Before condition whose protocol revealed that he had worked on the task for several minutes during the study period. The scoring procedure used on the 40 protocols was identical to that used for Experiment 1.

Reading Times

Table 8 shows the mean time subjects spent studying the documentation, as a function of condition. As the table indicates, subjects took more time to read the elaborated version than the unelaborated. They also took slightly more study time when they had prior knowledge of the tasks. Although these differences were not reliable, they are consistent with our speculation that subjects in the Elaborated-Before condition of Experiment 1 were hurt by insufficient time to both process the author-provided elaborations and generate their own.

In order to assess whether the one-hour time limit was itself too short, we noted how many subjects used all or most of the alloted time. The numbers in parentheses in Table 8 represent the percentage of subjects in each condition who studied the material for more than 50 minutes. As we expected, subjects studying the elaborated version of the manual required more time if they knew the task instructions in advance. Even though the mean reading times for the two elaborated cells look nearly identical, 60% of the subjects in the Elaborated-Before condition studied for longer than 50 minutes while only 30% of the subjects in the Elaborated-After condition did so.

TABLE 8

Mean Reading Time in minutes

(Self-Paced)

for Experiment 2*

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	49 (.6)	45 (.4)	47
	After	48 (.3)	40 (.1)	44
		49	43	

* The numbers in parentheses are the percentage of subjects in each cell who studied the manual for more than 50 minutes.

Performance on Task

Success on Tasks

Completeness scores were devised for the four subtasks in this study in the same mannner as in Experiment 1.

- Rename task. Subjects were awarded one point for discovering the correct new name for each file and another point for sucessfully renaming the files.
- Combine task. Subjects were awarded 1 point for combining the files in the right order.
- Make Directory task. Subjects were awarded two points for sucessfully creating two subdirectories, two points for copying the appropriate files into each one, and two points for deleting the copied files from the root directory.
- Remove Directory task. Subjects were awarded two points for deleting the files in two successively deeper subdirectories and two more points for removing each of these subdirectories.

Table 9 displays the mean *percentage of tasks successfully completed*, as a function of condition. In contrast to Experiment 1, the Before conditions now produce better performance than the After conditions. Like Experiment 1, the elaborated versions of the manual produced better performance: however, all of these effects are quite small, i.e., there are no significant main effects or interactions.

We also performed a 2x2 analysis of covariance on the completeness scores, with reading time as the covariate. The adjusted cell means from this analysis appear in parentheses in Table 9. The adjustments are quite small for the most part and do not produce any reliable main effects. However, the analysis revealed a slight negative correlation between reading time and ability to complete tasks successfully, $F(1,35)=3.02$, $p<.1$, $r = -0.01$.

TABLE 9

Mean proportion of tasks successfully completed

for Experiment 2*

INSTRUCTION	VERSION OF MANUAL: Elaborated	VERSION OF MANUAL: Unelaborated	
Before	.82 (.83)	.80 (.80)	.81 (.82)
After	.78 (.79)	.78 (.75)	.78 (.77)
	.80 (.81)	.79 (.78)	

* The numbers in parentheses are the means for each cell adjusted for the length of time the subjects studied their version of the manual.

Efficiency of task execution: number of steps

Table 10a presents the data for the mean number of steps needed to complete the four tasks, as a function of condition. The differences between the Before and After conditions and the Elaborated and Unelaborated conditions were not significant. The interaction, on the other hand, is significant $F(1,36) = 5.88$, $p < .05$. The Elaborated-After condition produces the best performance, while the Unelaborated-After condition produces the worst. As in Experiment 1, the Elaborated-After condition is significantly better than the other three, $t(36) = -3.18$, $p < .01$.

Table 10b displays the proportion of steps taken per completed task. As in the previous experiment, this measure compares subjects' efficiency on tasks they completed relative to other successful subjects (the ratio involves the mean number of steps needed to successfully complete a subtask). This corrected measure does not show a reliable advantage for the elaborated conditions; however, the interaction between version and instruction is again significant, $F(1,36) = 6.28$, $p < .05$. In this case, however, it is the Unelaborated-After that seems different (worse) than the other three conditions, $t(36) = 1.99$, $p < .10$.

Finally, we compared the number of steps each subject took to complete a subtask against the minimum number of steps required to complete that task. Table 10c presents the mean proportion of steps per completed task. As in the two previous analyses, this measure produced no reliable main effects, but the interaction between version and instruction was once again significant, $F(1,36) = 5.49$, $p < .05$.

TABLE 10a

Mean number of steps taken by subjects to perform all subtasks in Experiment 2

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	93	81.7	87.35
	After	66.7	100.5	83.60
		79.85	91.1	

TABLE 10b

Mean proportion of steps taken per successfully completed task (compared with the mean number of steps taken by those who successfully completed that task) for Experiment 2*

		VERSION OF MANUAL: Elaborated	VERSION OF MANUAL: Unelaborated	
INSTRUCTION	Before	1.02 (1.04)	.84 (.84)	.93 (.94)
	After	.89 (.90)	1.18 (1.15)	1.04 (1.03)
		.96 (.97)	1.01 (1.00)	

* The numbers in parentheses are the means for each cell adjusted for the length of time the subjects studied their version of the manual.

TABLE 10c

Efficiency on completed tasks: the proportion of steps subjects took to complete tasks relative to the minimum number of steps required to successfully complete those tasks, for Experiment 2

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	3.3	2.4	2.9
	After	2.4	3.6	3.0
		2.9	3.0	

Efficiency of task execution: time on task

The time needed to complete the tasks was the third measure of skill performance. Table 11a presents the mean time subjects spent working on the *specified tasks* (*i.e. the* time until they either successfully completed all the tasks or gave up on completing some or all of them), as a function of condition. Here the time taken to complete the four tasks is significantly shorter (20% faster) for subjects who studied the elaborated manual than for those who studied the unelaborated version. $F(1.36) = 4.64$, $p < .05$. Subjects in the Before conditions are 10% or 3 minutes faster than those in the After conditions; however this effect and the interaction are non-significant. Once again, the Elaborated-After condition is superior to the other three, $t(36) = -2.30$, $p < .05$, and the Unelaborated-After is much worse, $t(36) = 2.17$, $p < .05$.

Table 11b presents a slightly different picture of efficiency in terms of time spent on task. *This measure only includes times for subtasks that are successfully completed* and computes the proportion of time a subject spent completing a subtask relative to the mean of successful completion times for those tasks. Here there is a main effect of instruction, $F(1.36) = 4.43$, $p < .05$, such that subjects in the Before conditions complete tasks significantly faster than subjects in the After conditions. The advantage of the elaborated conditions is now non-significant because subjects in the Unelaborated-Before condition do so well. In contrast, the Unelaborated-After condition is the worst condition, producing a marginally significant interaction $F(1.36) = 3.47$, $p < .10$. Indeed the Unelaborated-After cell is significantly worse than the other three, $t(36) = 3.3$, $p < .01$.

TABLE 11a

Mean time (in minutes) taken by subjects

to perform all subtasks in Experiment 2

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	31.02	32.49	31.76
	After	28.81	40.17	34.49
		29.92	36.33	

TABLE 11b

Mean proportion of time taken per successfully completed task (compared with the mean time taken by those who successfully completed that task) for Experiment 2*

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	.97 (.97)	.92 (.92)	.95 (.95)
	After	.99 (.99)	1.25 (1.25)	1.12 (1.12)
		.98 (.98)	1.09 (1.09)	

* The numbers in parentheses are the means for each cell adjusted for the length of time the subjects studied their version of the manual.

Declarative Test

Table 12 presents the mean percent correct on the true/false test that subjects in the four conditions took after completing the skill test. There was a main effect of when subjects were given the task instructions, $F(1,36)=5.19$, $p<.05$, such that subjects performed best on the declarative test if they had not seen the task instructions prior to reading the material (the After condition). Apparently subjects in the Before condition who read selectively for the specified task could not perform as well on an unexpected test for which there was no prior knowledge of the questions that would be asked. There was no significant effect of the degree of elaborations, however the Elaborated-After was better than the other three conditions, $t(36)=2.28$, $p<.05$, suggesting that success with the skill task may have translated to the declarative task.

TABLE 12

Mean percent correct for true/false

test in Experiment 2

		VERSION OF MANUAL		
		Elaborated	Unelaborated	
INSTRUCTION	Before	.73	.69	.71
	After	.82	.78	.80
		.78	.74	

General Discussion

The data from the second study tend to support the findings of Experiment 1. First, skill performance is better for subjects who studied the elaborated version of the manual, especially if they did not have advance knowledge of the tasks they would have to perform. An over-all advantage for the elaborated version appears in the marginals for every table in this paper. Often this effect is weak because the Before conditions do not show this advantage for the elaborated version consistently. Second, we expected an advantage to accrue from knowing about the tasks prior to studying the manual. That effect emerged as well, although it too was weak. Prior knowledge of the tasks appears to be a mixed blessing: it greatly aids subjects studying the unelaborated version, while it tends to impede the performance of subjects studying a document that is already elaborated.

A different way to capture our results is to note that the aberrant cell in both studies is the Elaborated-Before condition. We found that, in general, prior knowledge of the task helps performance. We also found that, in general, author-provided elaborations help performance. However, when both are provided together in the Elaborated-Before condition, performance is worse than one would expect. In the second study, we attempted to change this pattern by reducing the memory burden in the Before conditions, viz., by allowing subjects to keep the task instructions while studying the manual. Although subjects in this condition now completed more of the task successfully, they did not maintain their advantage on other measures. If memory load had been a problem in Experiment 1, it clearly was not the primary reason why the Elaborated-Before condition was worse. We also tried to improve performance in that condition by giving subjects more study time. We raised the maximum amount of time allowed from 45 minutes to one hour. This may not have been enough additional time.

Indeed, six of the 10 subjects in the Elaborated-Before condition studied the manual for over 50 minutes, and of these four took all the time that was allowed.

From another perspective, our results are not that surprising. Consider what effect we thought prior knowledge of the task would have. We imagined that prior knowledge would allow subjects to selectively attend to the material within the document that is directly relevant to the task. Time or attention that would ordinarily be devoted to less relevant information could be reallocated to information that is essential for performing the specified set of tasks. This extra time might then be used both for elaborating on the task-relevant statements in the document and for rehearsing these critical facts. Although these reader-generated elaborations are intended to strengthen memory traces and create redundancies that mitigate against forgetting, they could well be counter-productive when elaborations are already present in the document.

Elaborations are often thought to be a good memory aid because they provide redundant memory traces. If any one fact is forgotten, there will be others that can be retrieved, or else, the relevant fact can be reconstructed from the various traces that are still available. Elaborations provide additional retrieval routes to the information by making more connections with prior knowledge. In spite of these potential benefits, elaborations might also have a detrimental effect on retrieval: there is considerable evidence that interference can occur from having too many related facts in memory. There may be a ceiling in maximal benefit due to elaborations and that once that level is exceeded, the detrimental effects of interference come into play. Ballstaedt and Mandl (1981) suggest that subjects who generate too few elaborations and subjects who generate too many have poorer retention than those who elaborate an intermediate amount. Perhaps that result is due to the interaction of redundancy and interference.

There is another reason why the Elaborated-Before condition might have produced relatively poor performance. Even with the additional study time, subjects may not have had sufficient time to read the entire manual and still have time to rehearse the task-relevant points. While reading the document the first time, these subjects may conjecture as to the relevance of a particular command for performing one of the tasks. However, rather than committing that command to memory, the subject might decide to scan the entire document to check that there is not a superior command described later. *The reader's intention would be to return to that command later if no better one presented itself.* The problem with such a strategy is that there probably was not enough time to make the planned second pass through the document, and the first pass was not thorough enough to give these subjects a deep understanding of everything they read. Subjects in the Unelaborated-Before condition might well adopt this same strategy; however, since there was so much less to read in their version, they were able to make that second pass through the manual and reap the benefits of focussing more attention on the information relevant to the task. Conversely, subjects in the After conditions are unlikely to adopt such a strategy because without advance knowledge of they task, they have no way of deciding which commands are more relevant than others. Their strategy on the first pass would be to devote equal attention to all the commands, and try to commit all of them to memory. For the subjects in the Elaborated-After condition, this pays off because they can take advantage of the author-provided elaborations. The subjects in the Unelaborated-After condition, however, are less fortunate. They do not have author-provided elaborations to help them attain a better understanding of the commands in general, and they cannot generate good elaborations on their own because they don't know what kind of tasks to expect.

Prior knowledge of the task only facilitated skill performance with unembellished manuals. Further, the results of the declarative test strongly suggest that prior knowledge of the procedural task hurt subsequent performance on the surprise declarative test. This is consistent with past research that found performance better for subjects who were told what questions to expect, but worse for those same subjects on unexpected questions (e.g., Frase 1975; Frase 1967). Conceivably, performance on unexpected skill tasks would also suffer in the Before condition.

In conclusion, we believe that we have found evidence that carefully chosen elaborations make manuals more effective for teaching people how to perform a technical skill. It is clear, however, that if a person knows in advance what tasks will need to be done, a short, summary-like manual is more appropriate. This result should not be surprising given our reasons for believing that elaborations would help skill performance but detract from declarative learning. We argued that the advantage of elaborations would come primarily from helping a user to figure out which of the various procedures to apply in a specific task situation. However, with prior knowledge of task demands, the difficulty of figuring out which of the studied procedures are appropriate is diminished. While reading the manual, the learner can figure out which procedures to use, and when and where to apply them. The elaborations that the learner generates will be closely related to the tasks, and far better than those that can be provided by the author of the manual, who cannot anticipate all possible tasks, let alone generate elaborations to handle them.

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APPENDIX

Contents

The Commands Taught in the User's Manual

COMMAND	FUNCTION
DIR	List the files in a directory
MKDIR	Create (or "make") a subdirectory
CHDIR	Change default directory assignment
RMDIR	Eliminate (or "remove") a subdirectory
TYPE	Display the contents of a file
RENAME	Change the name of a file
COPY	Create a duplicate copy of a file Combine or append files together Transer data between system devices
ERASE	Eliminate a file
----------	
^-Numlock	Freeze the screen
^-Break	Abort the current command
d:	Change the default drive assignment

Sample Declarative Test Questions
Experiment 1

Suppose that the following directory listing shows the contents of the root directory on the diskette in drive A on the computer. Answer each question as if you were starting with this set of files, and write your answer in the space provided.

```
Volume in drive A is unlabelled
Directory of A:\

MAIL      TXT    10368     1-20-83   9:00a
RANDOM  LSP     1613     3-14-83  11:54p
PROGRAMS   <DIR>         2-05-83  10:34a
PARSER   LSP      795     2-24-83   3:15p

        4 File(s) 141312 bytes free
```

1. What does the following command do?

   At the A> prompt, type:

   COPY RANDOM.LSP+PARSER.LSP PROGRAMS\PROG.LSP <CR>

2. Write a command (specifying parameters as necessary), which *would accomplish the following:*

   Create a subdirectory called "PROGS.LSP" in the "PROGRAMS" subdirectory.

Sample Declarative Test Questions
Experiment 2

1. The RMDIR command can be used to delete any directory, including the root directory. (True/False)

2. To signal the computer that you are specifying a path, you must begin the path by typing a backslash character. (True/False)

3. It is impossible to have two copies of the same file, by the same name, in the same directory. (True/False)

Excerpt from an On-Line Protocol (Unanalyzed)
of Subject's Interaction with the IBM-PC
Experiment 1

```
[12:09:23][12:09:27] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER          <DIR>       2-18-84   12:08p
SYS            <DIR>       2-18-84   12:10p
UNIT     IO         470   10-18-83    3:28p
UNIT     D          396   10-18-83    3:51p
UNIT     T          683   10-18-83    3:53p
MINCE    BAT         24   10-18-83    3:36p
        6 File(s)    149504 bytes free

[12:09:30][12:09:36] A>mkdir unit


[12:09:36][12:09:55] A>copy unit.* unit

UNIT.IO
UNIT.D
UNIT.T
        3 File(s) copied

[12:10:03][12:10:05] A>dir\unit


 Volume in drive A has no label
 Directory of  A:\unit

.              <DIR>       2-18-84   12:12p
..             <DIR>       2-18-84   12:12p
UNIT     IO         470   10-18-83    3:28p
UNIT     D          396   10-18-83    3:51p
UNIT     T          683   10-18-83    3:53p
        5 File(s)    145408 bytes free

[12:10:08][12:10:11] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER          <DIR>       2-18-84   12:08p
SYS            <DIR>       2-18-84   12:10p
UNIT           <DIR>       2-18-84   12:12p
UNIT     IO         470   10-18-83    3:28p
UNIT     D          396   10-18-83    3:51p
UNIT     T          683   10-18-83    3:53p
MINCE    BAT         24   10-18-83    3:36p
```

```
        7 File(s)    145408 bytes free

[12:10:14][12:10:21] A>del unit.*


[12:10:21][12:10:25] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER        <DIR>       2-18-84  12:08p
SYS          <DIR>       2-18-84  12:10p
UNIT         <DIR>       2-18-84  12:12p
MINCE    BAT        24  10-18-83   3:36p
        4 File(s)    148480 bytes free

[12:10:28][12:12:13] A>con:

Bad command or file name

[12:12:14][12:12:37] A>migrate.lst con:

Bad command or file name

[12:12:38][12:13:10] A>create migrate.lst

Bad command or file name

[12:13:11][12:13:15] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER        <DIR>       2-18-84  12:08p
SYS          <DIR>       2-18-84  12:10p
UNIT         <DIR>       2-18-84  12:12p
MINCE    BAT        24  10-18-83   3:36p
        4 File(s)    148480 bytes free

[12:13:18][12:13:26] A>del mince.bat


[12:13:26][12:13:30] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER        <DIR>       2-18-84  12:08p
SYS          <DIR>       2-18-84  12:10p
UNIT         <DIR>       2-18-84  12:12p
        3 File(s)    149504 bytes free
```

```
[12:13:33][12:14:15] A>copy con: migrate.lst

mince.bat
^Z

        1 File(s) copied

[12:14:42][12:14:44] A>dir


 Volume in drive A has no label
 Directory of  A:\

TIMER        <DIR>      2-18-84  12:08p
SYS          <DIR>      2-18-84  12:10p
UNIT         <DIR>      2-18-84  12:12p
MIGRATE  LST       11   2-18-84  12:16p
        4 File(s)   148480 bytes free

[12:14:47][12:14:52] A>type migrate.lst

mince.bat
```

Excerpt from an On-Line Protocol
After Analysis and Segmentation into Subtask
Experiment 1

```
<MKD Task>[Dir   ] 69 -( 71) [12:09:27] A>DIR
<MKD Task>[Mkdir ] 70 -( 72) [12:09:36] A>MKDIR UNIT
<MKD Task>[Copy  ] 71 -( 73) [12:09:55] A>COPY UNIT.* UNIT
<MKD Task>[Dir   ] 72 -( 74) [12:10:05] A>DIR\UNIT
<MKD Task>[Dir   ] 73 -( 75) [12:10:11] A>DIR
<MKD Task>[Delete] 74 -( 76) [12:10:21] A>DEL UNIT.*
<MKD Task>[Dir   ] 75 -( 77) [12:10:25] A>DIR
<MGT Task>[Misc  ] 76 -( 78) [12:12:13] A>CON:
<MGT Task>[Misc  ] 77 -( 79) [12:12:37] A>MIGRATE.LST CON:
<MGT Task>[Misc  ] 78 -( 80) [12:13:10] A>CREATE MIGRATE.LST
<MGT Task>[Dir   ] 79 -( 81) [12:13:15] A>DIR
<MGT Task>[Delete] 80 -( 82) [12:13:26] A>DEL MINCE.BAT
<MGT Task>[Dir   ] 81 -( 83) [12:13:30] A>DIR
<MGT Task>[Copy  ] 82 -( 84) [12:14:15] A>COPY CON: MIGRATE.LST
<MGT Task>[Dir   ] 83 -( 85) [12:14:44] A>DIR
<MGT Task>[Type  ] 84 -( 86) [12:14:52] A>TYPE MIGRATE.LST
```

Navy

1 Robert Ahlers
Code N711
Human Factors Laboratory
NAVTRAEQUIPCEN
Orlando, FL 32813

1 Dr. Ed Aiken
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Meryl S. Baker
Navy Personnel R&D Center
San Diego, CA 92152

1 Code N711
Attn: Arthur S. Blaiwes
Naval Training Equipment Center
Orlando, FL 32813

1 Dr. Richard Braby
TAEG
Naval Training Equipment Center
Orlando, FL 32813

1 Dr. Robert Breaux
NAVTRAEQUIPCEN
Code N-095R
Orlando, FL 32813

1 Dr. Richard Cantone
Navy Research Laboratory
Code 7510
Washington, DC 20375

1 Dr. Robert Carroll
NAVOP 115
Washington , DC 20370

1 Dr. Fred Chang
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Susan Chipman
Code 442PT
Office of Naval Research
800 N. Quincy St.
Arlington, VA 22217

1 Dr. Stanley Collyer
Office of Naval Technology
800 N. Quincy Street
Arlington, VA 22217

Navy

1 CDR Mike Curran
Office of Naval Research
800 N. Quincy St.
Code 270
Arlington, VA 22217

1 Dr. Charles E. Davis
Personnel and Training Research
Office of Naval Research (Code 442PT)
800 North Quincy Street
Arlington, VA 22217

1 Edward E. Eddowes
CNATRA N301
Naval Air Station
Corpus Christi, TX 78419

1 Dr. Marshall J. Farr
Director, Psychological Sciences
Office of Naval Research (Code 442)
800 North Quincy Street
Arlington, VA 22217

1 Dr. Jude Franklin
Code 7510
Navy Research Laboratory
Washington, DC 20375

1 Dr. Jim Hollan
Code 14
Navy Personnel R & D Center
San Diego, CA 92152

1 Dr. Ed Hutchins
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Norman J. Kerr
Chief of Naval Technical Training
Naval Air Station Memphis (75)
Millington, TN 38054

1 Dr. Peter Kincaid
Training Analysis & Evaluation Group
Dept. of the Navy
Orlando, FL 32813

1 Dr. William L. Maloy (02)
Chief of Naval Education and Training
Naval Air Station
Pensacola, FL 32508

Navy

1 Dr. Joe McLachlan
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. James McMichael
Navy PersonnelR&D Center
San Diego, CA 92152

1 Dr William Montague
NPRDC Code 13
San Diego, CA 92152

1 Office of Naval Research
Code 433
800 N. Quincy SStreet
Arlington, VA 22217

1 Director
Engineering Psychology Group
Code 442EP
Office of Naval Research
800 N. Quincy Street
Arlington, VA 22217

1 Organizational Effectiveness
Research Group, Code 442OE
Office of Naval Research
Arlington, VA 22217

1 Psychologist
ONR Branch Office
1030 East Green Street
Pasadena, CA 91101

1 LT Frank C. Petho, MSC, USN (Ph.D)
CNET (N-432)
NAS
Pensacola, FL 32508

1 Dr. Gil Ricard
Code N711
NTEC
Orlando, FL 32813

1 Dr. Paul B. Schneck
Office of Naval Research
Code 433
800 N. Quincy
Arlington, VA 22217

1 Dr. Michael G. Shafto
ONR Code 442PT
800 N. Quincy Street
Arlington, VA 22217

Navy

1 Dr. Douglas Smith
Office of Naval Research
Code 433
800 N. Quincy
Arlington, VA 22217

1 Dr. Robert G. Smith
Office of Chief of Naval Operations
OP-987H
Washington, DC 20350

1 Dr. Alfred F. Smode
Senior Scientist
Code 7B
Naval Training Equipment Center
Orlando, FL 32813

1 Dr. Richard Sorensen
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Frederick Steinheiser
CNO - OP115
Navy Annex
Arlington, VA 20370

1 Dr. Thomas Sticht
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Martin A. Tolcott
Leader, Psychological Sciences Division
Office of Naval Research
800 N. Quincy St.
Arlington, VA 22217

1 Dr. James Tweeddale
Technical Director
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. Nick Van Matre
CNET
NAS
Pensacola, FL 32508

1 Dr. Wallace Wulfeck, III
Navy Personnel R&D Center
San Diego, CA 92152

Navy

1 Dr. Steven Zornetzer
Associate Director for Life Sciences
Office of Naval Research
800 N. Quincy St.
Arlington, VA 22217

Army

1 Technical Director
U. S. Army Research Institute for the
Behavioral and Social Sciences
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Beatrice J. Farr
U. S. Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Milton S. Katz
Training Technical Area
U.S. Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Harold F. O'Neil, Jr.
Director, Training Research Lab
Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Commander, U.S. Army Research Institute
for the Behavioral & Social Sciences
ATTN: PERI-BR (Dr. Judith Orasanu)
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Joseph Psotka, Ph.D.
ATTN: PERI-1C
Army Research Institute
5001 Eisenhower Ave.
Alexandria, VA 22333

1 Dr. Robert Sasmor
U. S. Army Research Institute for the
Behavioral and Social Sciences
5001 Eisenhower Avenue
Alexandria, VA 22333

1 DR. ROBERT J. SEIDEL
US Army Research Institute
5001 Eisenhower Ave.
300 N. WASHINGTON ST.
Alexandria, VA 22333

Air Force

1 U.S. Air Force Office of Scientific Research
Life Sciences Directorate, NL
Bolling Air Force Base
Washington, DC 20332

1 Dr. Earl A. Alluisi
HQ, AFHRL (AFSC)
Brooks AFB, TX 78235

1 Bryan Dallman
AFHRL/LRT
Lowry AFB, CO 80230

1 Dr. Genevieve Haddad
Program Manager
Life Sciences Directorate
AFOSR
Bolling AFB, DC 20332

1 Dr. T. M. Longridge
AFHRL/OTE
Williams AFB, AZ 85224

1 Dr. John Tangney
AFOSR/NL
Bolling AFB, DC 20332

1 Dr. Joseph Yasatuke
AFHRL/LRT
Lowry AFB, CO 80230

Departrent of Defense

1 Dr. Craig I. Fields
Advanced Research Projects Agency
1400 Wilson Blvd.
Arlington, VA 22209

1 Military Assistant for Training and Personnel Technology
Office of the Under Secretary of Defens for Research & Engineering
Room 3D129, The Pentagon
Washington, DC 20301

1 Major Jack Thorpe
DARPA
1400 Wilson Blvd.
Arlington, VA 22209

1 Dr. Robert A. Wisher
OUSDRE (ELS)
The Pentagon, Room 3D129
Washington, DC 20301

Civilian Agencies

1 Edward Esty
Department of Education, OERI
MS 40
1200 19th St., NW
Washington, DC 20208

1 Dr. Arthur Melmed
724 Brown
U. S. Dept. of Education
Washington, DC 20208

1 Dr. Andrew R. Molnar
Office of Scientific and Engineering
Personnel and Education
National Science Foundation
Washington, DC 20550

1 Dr. Ramsay W. Selden
National Institute of Education
1200 19th St., NW
Washington, DC 20208

1 Dr. Edward C. Weiss
National Science Foundation
1800 G Street, NW
Washington, DC 20550

1 Dr. Frank Withrow
U. S. Office of Education
400 Maryland Ave. SW
Washington, DC 20202

1 Dr. Joseph L. Young, Director
Memory & Cognitive Processes
National Science Foundation
Washington, DC 20550

Private Sector

1 Dr. John R. Anderson
Department of Psychology
Carnegie-Mellon University
Pittsburgh, PA 15213

1 Patricia Baggett
Department of Psychology
University of Colorado
Boulder, CO 80309

1 Eva L. Baker
Director
UCLA Center for the Study of Evaluation
145 Moore Hall
University of California, Los Angeles
Los Angeles, CA 90024

1 Mr. Avron Barr
Department of Computer Science
Stanford University
Stanford, CA 94305

1 Dr. John Black
Yale University
Box 11A, Yale Station
New Haven, CT 06520

1 Dr. John S. Brown
XEROX Palo Alto Research Center
3333 Coyote Road
Palo Alto, CA 94304

1 Dr. Bruce Buchanan
Department of Computer Science
Stanford University
Stanford, CA 94305

1 Dr. Jaime Carbonell
Carnegie-Mellon University
Departrent of Psychology
Pittsburgh, PA 15213

1 Dr. Pat Carpenter
Department of Psychology
Carnegie-Mellon University
Pittsburgh, PA 15213

1 Dr. Davida Charney
Department of Psychology
Carnegie-Melon University
Schneley Park
Pittsburgh, PA 15213

Private Sector

1 Eugene Charniak
Department of Computer Science
Brown University
Providence, RI 02912

1 Dr. Micheline Chi
Learning R & D Center
University of Pittsburgh
3939 O'Hara Street
Pittsburgh, PA 15213

1 Dr. William Clancey
Department of Computer Science
Stanford University
Stanford, CA 94306

1 Dr. Michael Cole
University of California
at San Diego
Laboratory of Comparative
Human Cognition - D003A
La Jolla, CA 92093

1 Dr. Allan M. Collins
Bolt Beranek & Newman, Inc.
50 Moulton Street
Cambridge, MA 02138

1 Dr. Emmanuel Donchin
Department of Psychology
University of Illinois
Champaign, IL 61820

1 Dr. Thomas M. Duffy
Department of English
Carnegie-Mellon University
Schenley Park
Pittsburgh, CA 15213

1 Dr. Anders Ericsson
Department of Psychology
University of Colorado
Boulder, CO 80309

1 Dr. Paul Feltovich
Department of Medical Education
Southern Illinois University
School of Medicine
P.O. Box 3926
Springfield, IL 62708

Private Sector

1 Mr. Wallace Feurzeig
Department of Educational Technology
Bolt Beranek & Newman
10 Moulton St.
Cambridge, MA 02238

1 Dr. Dexter Fletcher
University of Oregon
Department of Computer Science
Eugene, OR 97403

1 Dr. John R. Frederiksen
Bolt Beranek & Newman
50 Moulton Street
Cambridge, MA 02138

1 Dr. Michael Genesereth
Department of Computer Science
Stanford University
Stanford, CA 94305

1 Dr. Dedre Gentner
Bolt Beranek & Newman
10 Moulton St.
Cambridge, MA 02138

1 Dr. Robert Glaser
Learning Research & Development Center
University of Pittsburgh
3939 O'Hara Street
PITTSBURGH, PA 15260

1 Dr. Marvin D. Glock
217 Stone Hall
Cornell University
Ithaca, NY 14853

1 Dr. Josph Goguen
SRI International
333 Ravenswood Avenue
Menlo Park, CA 94025

1 DR. JAMES G. GREENO
LRDC
UNIVERSITY OF PITTSBURGH
3939 O'HARA STREET
PITTSBURGH, PA 15213

1 Dr. Henry M. Halff
Halff Resources
4918 33rd Road, North
Arlington, VA 22207

Private Sector

1 Dr. Reid Hastie
Department of Psychology
Northwestern University
Evanston, IL 60201

1 Dr. Barbara Hayes-Roth
Department of Computer Science
Stanford University
Stanford, CA 95305

1 Dr. Joan I. Heller
Graduate Group in Science and
Mathematics Education
c/o School of Education
University of California
Berkeley, CA 94720

1 Dr. James R. Hoffman
Department of Psychology
University of Delaware
Newark, DE 19711

1 Melissa Holland
American Institutes for Research
1055 Thomas Jefferson St., N.W.
Washington, DC 20007

1 Dr. Kristina Hooper
Corporate Research, ATARI
1196 Borregas
Sunnyvale, CA 94086

1 Dr. Marcel Just
Department of Psychology
Carnegie-Mellon University
Pittsburgh, PA 15213

1 Dr. David Kieras
Department of Psychology
University of Arizona
Tuscon, AZ 85721

1 Dr. Walter Kintsch
Department of Psychology
University of Colorado
Boulder, CO 80302

1 Dr. Pat Langley
The Robotics Institute
Carnegie-Mellon University
Pittsburgh, PA 15213

Private Sector

1 Dr. Jill Larkin
Departrent of Psychology
Carnegie Mellon University
Pittsburgh, PA 15213

1 Dr. Alan Lesgold
Learning R&D Center
University of Pittsburgh
3939 O'Hara Street
Pittsburgh, PA 15260

1 Dr. Jim Levin
University of California
at San Diego
Laboratory fof Comparative
Human Cognition - D003A
La Jolla, CA 92093

1 Dr. Marcia C. Linn
Lawrence Hall of Science
University of California
Berkeley, CA 94720

1 Dr. Don Lyon
P. O. Box 44
Higley , AZ 85236

1 Dr. Jay McClelland
Departrent of Psychology
MIT
Cambridge, MA 02139

1 Dr. James R. Miller
Computer*Thought Corporation
1721 West Plano Highway
Plano, TX 75075

1 Dr. Allen Munro
Behavioral Technology Laboratories
1845 Elena Ave., Fourth Floor
Redondo Beach, CA 90277

1 Dr. Donald A Norman
Cognitive Science, C-015
Univ. of California, San Diego
La Jolla, CA 92093

1 Dr. Jesse Orlansky
Institute for Defense Analyses
1801 N. Beauregard St.
Alexandria, VA 22311

Private Sector

1 Dr. Nancy Pennington
University of Chicago
Graduate School of Business
1101 E. 58th St.
Chicago, IL 60637

1 Dr. Ann Piestrup
THE LEARNING COMPANY
545 Middlefield Road, Suite 170
Menlo Park, CA 94025

1 Dr. Steven E. Poltrock
Bell Laboratories 2D-444
600 Mountain Ave.
Murray Hill, NJ 07974

1 Dr. Lynne Reder
Department of Psychology
Carnegie-Mellon University
Schenley Park
Pittsburgh, PA 15213

1 Dr. Fred Reif
Physics Department
University of California
Berkeley, CA 94720

1 Dr. Lauren Resnick
LRDC
University of Pittsburgh
3939 O'Hara Street
Pittsburgh, PA 1521

1 Dr. Jeff Richardson
Denver Research Institute
University of Denver
Denver, CO 80208

1 Mary S. Riley
Program in Cognitive Science
Center for Human Information Processing
University of California, San Diego
La Jolla, CA 92093

1 Dr. Andrew M. Rose
American Institutes for Research
1055 Thomas Jefferson St. NW
Washington, DC 20007

1 Dr. William B. Rouse
Georgia Institute of Technology
School of Industrial & Systems
Engineering
Atlanta, GA 30332

Private Sector

1 Dr. Roger Schank
Yale University
Department of Computer Science
P.O. Box 2158
New Haven, CT 06520

1 Dr. Walter Schneider
Psychology Department
603 E. Daniel
Champaign, IL 61820

1 Dr. Alan Schoenfeld
Mathematics and Education
The University of Rochester
Rochester, NY 14627

1 Dr. Ted Shortliffe
Computer Science Department
Stanford University
Stanford, CA 94305

1 Dr. Edward E. Smith
Bolt Beranek & Newman, Inc.
50 Moulton Street
Cambridge, MA 02138

1 Dr. Eliott Soloway
Yale University
Department of Computer Science
P.O. Box 2158
New Haven, CT 06520

1 Dr. Kathryn T. Spoehr
Psychology Department
Brown University
Providence, RI 02912

1 Dr. Robert Sternberg
Dept. of Psychology
Yale University
Box 11A, Yale Station
New Haven, CT 06520

1 Dr. Albert Stevens
Bolt Beranek & Newman, Inc.
10 Moulton St.
Cambridge, MA 02238

1 Dr. David Stone
KAJ Software, Inc.
3420 East Shea Blvd.
Suite 161
Pheonix, AZ 85028

Private Sector

1 DR. PATRICK SUPPES
INSTITUTE FOR MATHEMATICAL STUDIES IN
THE SOCIAL SCIENCES
STANFORD UNIVERSITY
STANFORD, CA 94305

1 Dr. Kikumi Tatsuoka
Computer Based Education Research Lab
252 Engineering Research Laboratory
Urbana, IL 61801

1 Dr. Perry W. Thorndyke
Perceptronics, Inc.
545 Middlefield Road, Suite 140
Menlo Park, CA 94025

1 Dr. Douglas Towne
Univ. of So. California
Behavioral Technology Labs
1845 S. Elena Ave.
Redondo Beach, CA 90277

1 Dr. Kurt Van Lehn
Xerox PARC
3333 Coyote Hill Road
Palo Alto, CA 94304

1 Beth Warren
Bolt Beranek & Newman, Inc.
50 Moulton Street
Cambridge, MA 02138

1 Dr. Keith T. Wescourt
Perceptronics, Inc.
545 Middlefield Road, Suite 140
Menlo Park, CA 94025

1 Dr. Mike Williams
IntelliGenetics
124 University Avenue
Palo Alto, CA 94301